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THE FOUNDRYMEN'S ASSOCIATIONS.

At the March meeting of the Foundrymen's Associations a paper on "Annealing Malleable Cast Iron" was read by Mr. George C. Davis, of Easton, Pa., as follows:

ANNEALING MALLEABLE CAST IRON.

By GEORGE C. DAVIS, Easton, Pa.

Two statements have been frequently made concerning the changes resulting from annealing that have not attracted the attention they deserve. The first is that malleable iron gains in weight, and the second is that it expands about $\frac{1}{8}$ " per foot. Now, the question naturally arises, Is there actually any expansion, or is it merely concretion or building up of the castings from the surrounding packing? Some thin castings were given a decarbonizing annealing in mill scale packing in a very hot oven. The results are given in table No. 1, the weights being stated in grams:

TABLE NO. 1—SHOWING GAIN IN WEIGHT.

Hard Iron.	Annealed.	% Gained.	2d Anneal.	% Gained.
40.1575	42.4900	5.81	42.5460	.13
44.8700	47.5465	5.96	47.5885	.09
38.4440	41.0430	6.76	41.0885	.11

We deduce from these figures that, under these conditions, there is an average net gain of 6.29 %, or, allowing for loss of carbon (approximately 3.40 in this case), that there is an actual concretion of 9.69 % of the weight of the casting.

Next a number of small castings were annealed in a packing not containing any appreciable amount of iron. Sand was the packing used, and the results are given in table No. 2:

TABLE NO. 2—SHOWING EFFECT OF ANNEALING IN SAND.

Hard Iron.	Annealed Iron.	% Loss.
72.657	70.458	3.02
85.324	81.7810	4.15
75.4924	71.0820	5.84
41.6035	39.2855	5.56

These castings were covered with a thick, soft scale, and part of the loss was due to this, while an examination showed that there had also been a partial oxidation of the carbon. The pot was a small one and the castings rather more exposed to the action of the air than they would have been in a larger pot.

For a third experiment a set of bars, each 12" long by 1" wide, and of different thickness from $\frac{1}{8}$ " to $\frac{3}{4}$ ", were cast from the same heat. There were four bars in each set, and the results are as uniform as could be expected. I selected in each case the bar showing the nearest to the average, and the results are given in table No. 3. The first line represents the annealed bar and the second the unannealed in each case. The dimensions are given in inches:

TABLE No. 3—SHOWING EXPANSION.	Length.	Breadth.	Thickness.
$\frac{1}{8}$ " Bar	11.832	1.050	.143
	11.768	1.037	.132
$\frac{1}{4}$ " Bar	11.930	1.031	.267
	11.784	1.015	.254
$\frac{3}{8}$ " Bar	11.927	1.043	.392
	11.757	1.020	.382
$\frac{1}{2}$ " Bar	12.093	1.021	.509
	11.909	1.000	.497
$\frac{5}{8}$ " Bar	11.942	1.032	.633
	11.742	1.007	.615
$\frac{3}{4}$ " Bar	11.964	1.030	.749
	11.769	1.005	.732

In table No. 4 I have given the figures showing increase in weight and diffusion of carbon on the above bars. The iron analyzed, silicon .60, sulphur .047, and total carbon 3.23, except in the case of the $\frac{1}{8}$ " bars, which were poured from another ladle, and contained 3.18 % carbon.

TABLE NO. 4—SHOWING CHANGES IN WEIGHT.

	Unannealed.	Annealed.	Gain %	Loss Carbon.	Concretion
$\frac{1}{8}$ " Bar	204.9	211.6	3.27	2.90	6.17
$\frac{1}{4}$ " Bar	377.8	384.6	1.80	1.97	3.77
$\frac{3}{8}$ " Bar	555.1	561.6	1.17	1.43	2.60
$\frac{1}{2}$ " Bar	735.2	740.2	.68	1.28	1.96
$\frac{5}{8}$ " Bar	882.5	890.1	.86	1.27	2.13
$\frac{3}{4}$ " Bar	1068.8	1076.4	.71	1.18	1.89

An examination of these tables shows that the changes result from complex causes. Taking the extreme cases, the $\frac{1}{8}$ " bar increased in length about one-third as much as the $\frac{3}{4}$ " bar. The cubic contents of the $\frac{1}{8}$ " bar, based on the above dimensions, are 1.61 and 1.776 inches, a gain of 10.29 %, while the concretion was 6.17 %. In the $\frac{3}{4}$ " bar we obtain 8.657 and 9.23 or an increase of 6.60 %, while the concretion was 1.89 %. Now, in the first case the increased volume appears to have been largely due to concretion, while in the second case the remaining graphitic carbon may be the cause. In the white iron practically all the carbon is in combination. During the annealing this carbide is decomposed and the carbon nearly all removed, while in the $\frac{3}{4}$ " bar two-thirds of the original carbon remains as graphite. It should be noted that the measurements taken on the light bars cannot be so reliable as those on the heavier sections. The reason for this is the warping of the light sections during annealing. The carbon determinations were made on samples taken from a cross-section in the center of the bar.

Just what reaction occurs between the carbon of the casting and the iron oxide of packing there is not sufficient data to determine, but in all probability it is complex. We know that considerable carbon oxide escapes from the pots during annealing,

and that both ferric and ferrous oxides are present in the packing. A comparison of the results in tables No. 1 and No. 3 shows that the concretion is greater if the annealing is done at a higher temperature. This illustrates the difficulty in making any general statement covering the case. All we can safely assert is that the carbon as it diffuses reduces the adjacent packing, which at the annealing heat welds onto the casting.

TABLE NO. 5—DIFFUSION OF SULPHUR.

	White Iron.	Annealed.		White Iron.	Annealed
$\frac{1}{8}$ " Bar047	.072	$\frac{5}{8}$ " Bar047	.062
$\frac{1}{4}$ " Bar047	.066	$\frac{3}{4}$ " Bar047	.058
$\frac{3}{8}$ " Bar047	.060	$\frac{1}{2}$ " Bar, outside	.047	.082
$\frac{1}{2}$ " Bar047	.058	$\frac{1}{2}$ " Bar, inside.	.047	.046

These results show that sulphur diffuses, and, like carbon, it varies with the cross-section of the casting, other conditions being the same. That the gain in sulphur is near the surface is shown by the results on the $\frac{1}{4}$ " bar. While the center remained unchanged, the outside sample taken 1-50" increased .035. It will probably be a surprise to many that there is so large a gain in this element. Much of the packing is made from mill scale, from common grades of wrought iron. This was originally made from forge pig containing .10 % or often more in sulphur. In the heating furnace the muck bar is exposed to the gases of combustion and a further absorption takes place. Samples of mill scale from different works gave an analysis of .17 % and .26 % for wrought iron scale, and .061 % and .084 % for steel scale. In the decarbonizing annealing of thin castings it is necessary to have a packing as low in sulphur as possible and free from foreign matter. A pitted surface will often result from poor packing caused both by the diffusion of sulphur and small particles of foreign matter becoming imbedded in the iron that builds up the casting. In the case of castings that are to be polished or plated, this pitted surface is objectionable, as it renders the finishing operations difficult. Iron ores very low in sulphur can be obtained, but the present high price of iron is almost prohibitive. Annealing in ore requires rather different treatment, as the ores

are chemically more active and the castings lose, rather than gain, owing to oxidation of the iron. Laudig (Trans. Amer. Ins. Mining Eng.) speaks of this fact in connection with the reducibility of ores in a blast furnace, and has shown that there is a wide difference in the reducibility of apparently similar oxides of iron. The length of these test bars in these experiments was measured on an improvised scale. This was made by cutting a 2-inch micrometer in half and bolting each on an iron frame, so that at zero the distance was $11\frac{3}{4}$ ". As the micrometer was graduated from 1" to 2", we were thus enabled to read any variation between $11\frac{3}{4}$ " and $12\frac{3}{4}$ ".

Richard Moldenke presented the following discussion before the Pittsburg meeting:

Mr. Davis presents some interesting matter in his paper, and I depart from my usual custom of refraining from discussing "malleable," on account of the various statements made recently concerning it, which will in time react upon the manufacturer to his material disadvantage. The expansion taking place during anneal is well known and allowed for in making the patterns; but the fact that under certain conditions there is an increase in weight is not generally known outside of those well posted in the trade. It may be taken as a general rule that the contraction of the chilling irons used for "malleable" is $\frac{1}{4}$ -inch to the foot. In the anneal an expansion of $\frac{1}{8}$ -inch to the foot takes place, the net result being the ordinary contraction allowed for gray iron. Patterns are therefore made in the usual manner, the special art of the "malleable" pattern maker being shown in the gating, chill arrangements, etc.

The contraction and expansion in "malleable" are not regular by any means, as the results of thousands of measurements have shown me. The reasons would seem to lie in the great differences due to : 1, slight changes in the chemical composition; 2, casting temperature changes; 3, shrinkages in the interior of the work. The test bar of "malleable" which does not show some interior shrinkage planes at right angles to its length and width is a *rara avis*. You will please note that I use the term "shrinkage"

for the unsound portions produced in the casting during the act of setting, while the reduction in length is designated as the contraction; for just look at the expansion of cast iron under heat a moment. Even with the imperfect knowledge we have of the expansion of metals under high temperatures, we know that a piece of cast iron increases about 1-60 of its length in heating from ordinary temperatures to the melting point. Reverse this and we see that a casting left free to contract in the mold will lose roughly 1-5 inch to the foot. It would seem then that which is usually called shrinkage is a simple contraction due to cooling.

To return to the subject. The contraction in the "hard" casting may vary from a little over $\frac{1}{8}$ -inch to nearly 5-16 inch. In the anneal this may or may not be brought back to the exact $\frac{1}{8}$ -inch. A long malleable casting can therefore show some unexpected features. The remarkable change in the casting due to the annealing process will some day be more fully understood. My own observations have been that the removal of the carbon in the skin is but a small matter when compared with the interior change of combined carbon to what Prof. Ledebur has called "temper carbon" (temperguss—malleable casting). A malleable casting of the proper composition should contain no graphite strictly speaking. The carbon is not crystallized, but is a soft amorphous variety similar to lamp black. If caught in the first stages of formation it may be readily removed with a needle when under the microscope.

How the carbon leaves the skin of a casting during anneal is something we are not acquainted with sufficiently as yet, and in furnace iron but little leaves it anyhow. A piece, say, $\frac{1}{2}$ -inch thick has the full amount remaining in the center; though cupola iron, requiring a much higher annealing heat, will necessarily have the loss in carbon accentuated somewhat. I have always noticed that when a heavy piece of "malleable" has not annealed properly, there is also very little expansion—a fact easily verified by trying to anneal a heavy piece of cupola iron at furnace iron temperature. One interesting point may be stated here which shows that if the carbon is removed by outside agencies

it can only be carbonic oxide with but very little carbonic acid. I have repeatedly found a thin layer of lamp black on partly annealed castings when so packed in air-tight receptacles, removed from all oxidizing influences, that only the little air present in the first place could act upon them.

To sum up: The change from the dense iron carbides of the hard castings to the open malleableized structure after anneal means the exertion of a powerful expansive force creating inter-crystalline spaces for the disposal of the temper carbon which would otherwise have no place. The expansion of malleable is probably due to these forces and not to iron added from the scale.

I am inclined to take issue with Mr. Davis on what he calls the concretion of malleable castings. If he can really gain about 6 per cent of iron during the anneal he will make our gray iron founders envious, for he really recoups himself for the greater part of his melting loss. Now, that a casting can actually weigh more after the anneal than before is a fact; but I have always considered it an unfortunate one when I met it, for in my estimation the added weight is not due to iron welded on from the scale, but to the oxidation of the casting itself, which means weakness. A glance at Table 1 of the paper shows this. Why was only 0.10 per cent of iron (?) added to the second anneal of a piece which gained 60 times as much from the first? By the way, $6\frac{1}{2}$ per cent added to the weight of a casting is quite an appreciable quantity. Too bad that the dimensions of the pieces are not given so that the added thickness might be calculated.

Now as to the next point of difference: I may as well state frankly that every carbon analysis of malleable castings which I have seen published is unreliable. I was once given a piece of malleable analyzed by two well known chemists, one of whom found .27 total carbon, the other 3.99. Both results were incorrect, for one took his sample from the skin, the other from the center of the fracture. In fact, an absolutely correct analysis cannot be made, though it should approximate the truth very closely. I will explain what I mean, by one example. A piece of $\frac{3}{4}$ -inch "malleable," ends broken off, was placed in the shaper and 1-16-

inch cuts taken off. The first cut ran 0.16 total carbon; the second 0.65 per cent; the next 1.84 per cent, the next 3.97 per cent, and the next 4.05 per cent. The hard casting contained 4.08 per cent total carbon. On the face of this, of what good is an analysis of "malleable" showing a total loss of carbon due to annealing unless the whole of a sample bored through the test piece is used for the determination? Boring a lot of holes, mixing the drillings, and selecting samples simply will not do. I am afraid, therefore, that the figures given by Mr. Davis incline toward too great a removal of carbon, and the assumption of a loss of 3.40 per cent is far too high a guess.

When Mr. Davis speaks of the diffusion of carbon in annealing I think he comes nearer the real truth of the process than he may be aware of. In my experience with over 100,000 tons of malleable castings I have gained a wholesome respect for oxygen, as you may have observed in my writings on gray iron. Just try to anneal a piece of gray iron in the regular process for "malleable" and note the beautiful banded "calico" structure of the rotten specimen of iron that comes out. Now, here is a good example of oxygen getting into and ruining a piece of iron with nearly 4 per cent carbon. Why did not the oxygen take the carbon and leave the iron alone? Well, it did not: and here I have always had a lurking suspicion that once oxygen got into iron it was mighty hard to get out again. What then becomes of the universally accepted theory that oxygen enters the malleable casting to remove its carbon? It always seemed to me that carbon diffusing out did have some chance, for when oxygen does get into a malleable casting we at once recognize its effect by the white crystalline rim of "burnt" malleable iron.

I heartily agree with Mr. Davis on the sulphur question, though I was seldom troubled with it, having a better grade of "scale" to deal with. His results would indicate that sulphur can readily infuse into the iron but cannot diffuse out, varying in this respect from Mr. Campbell's well-known experiments on steel.

Mr. Davis' interesting paper shows us how little we really

know about the internal economy of our beloved cast iron, and incidentally what a fascination it is to delve into the mysteries of even so simple-looking an article of our daily life.

Edward Kirk followed with some remarks as to what constituted a competent chemist for foundries, in which he said:

When visiting foundries in different sections of the country I have learned that but a small percentage of them employ chemists, and the reason is largely found in the system adopted by the advocates of chemistry, in placing the matter before the foundrymen. This has been done upon the theory that it is only necessary for the founder to have an analysis of his irons to enable him to produce castings of any degree of hardness, softness and strength desired, which in many instances has not been the case. This system seems to me to be entirely wrong and certainly has been productive of prejudice against the employment of chemists in foundries. By this system the founder is compelled to make his mixture from analysis, something in which he is not experienced and is responsible for results.

The question that many founders have asked is, "Why should we employ a chemist when we have the same work to do and responsibility as without one and no greater certainty of results?" It is a well established fact that the quality of an iron may greatly deteriorate when melted in a cupola by improper melting, and the result indicated by analysis may be entirely destroyed in this way. With the present system the science of chemistry is placed entirely at the mercy of the melter, who can destroy its aim at will, and who frequently does destroy it through ignorance of the proper management of a cupola or prejudice against chemistry. To relieve the founder of responsibility in mixing irons and to make the chemist of greater value to him, and also to place the latter in a position independent of the melter, and make him master of his science, I propose that a chemist be given full charge of the mixing of the iron and of the cupola and melter, and that he be held responsible for the results of his analysis at the cupola spout. To prepare the chemist for the work, I propose that a course of practical foundry chemistry be established by colleges and scien-

tific schools giving a course of metallurgy, to combine this with mechanical training that will make the chemist a practical melter, and an expert on cupola management as well as a chemist. I further propose that when he is employed in the foundry, he be given full charge of the cupola, and the mixing and melting of the iron.

I do not propose that the chemist do the laborious work of melting, but that he be given charge of the cupola and the melter with the authority to discharge the latter if he does not follow his instructions in the cupola management. By this system the cost of melting in many foundries could be reduced to such an extent as would more than pay the salary of the chemist. The results achieved would be more satisfactory melting and better castings. There would be an inducement to the founder to employ chemists and more advancement would be made in foundry chemistry in one year than has been made in the last 10 under the present system.

I desire to place this matter before the foundrymen's associations for their consideration, as a practical means of increasing the employment of chemists in foundries.

An animated discussion followed the reading of the paper. A number of foundrymen strongly asserted their opposition to the employment of chemists in their plants for the reason that chemists are not acquainted with foundry practice and are often at sea when called upon to make an analysis of iron and indicate the results that will be produced when different brands of iron are used in various proportions.

Dr. Moldenke championed the cause of the chemists, arguing that the chemist should be taken into the foundry and should be taught the methods in vogue, so that at some later date his scientific knowledge of irons could be of use to the foundryman. He favored giving the chemist full charge of the cupola as well as the mixing of the iron, but he did not believe that he should be clothed with this authority until after he had proved his usefulness, and had become conversant with the foundry practice.

E. A. Kebler then cited an instance that came to his notice of the employment of a chemist. He said that a certain foundry

had been newly placed in operation. A chemist was employed at the behest of the president of the company. When the chemist was introduced to the superintendent the latter asked him what he knew about foundry practice and the various kinds of iron that enter into various mixtures. The chemist replied that it was all foreign to him and he frankly acknowledged that he knew nothing about making the mixtures. The superintendent went directly to the president and said that the chemist would be of no use to him; but the head of the company still had faith in the scientific man and told the superintendent to teach him matters that he was unfamiliar with. After a month's experience the chemist began to get an inkling of what the foundry work was and some of his analyses began to prove useful. Shortly afterward a carload of iron was received at the foundry, which had a lustrous color, and the superintendent was congratulating himself on getting such good material. He told the president that he was particularly fortunate in securing such a good grade of iron. A sample was given to the chemist to analyze and he found that the iron was not what the superintendent believed it to be. In fact, he found it to be of very inferior quality and very hard. He told the superintendent that he had better go slow about using it, as he found it to be very hard. This is where the superintendent had a grievance. Right here his knowledge of irons by fracture and color clashed with the knowledge gained by the chemist by analysis. He went to the president with his grievance and told him that he believed the chemist did not know what he was talking about. A pig of the iron was used with other components of the mixture and the casting made therefrom was found to be very hard, just as the chemist predicted. The superintendent was forced to acknowledge that the chemist was right, and gave the latter a little more leeway. Shortly afterward another carload of iron was received, which the superintendent immediately pronounced very poor. The chemist, however, analyzed it to be particularly good, and the castings made therefrom were found to be the best ever turned out. Mr. Kebler

said that now the chemist and the superintendent work together, with the result that very fine castings are produced.

After the discussion a committee was appointed, consisting of Wm. Yagle, Thomas D. West and W. B. Rodgers, to consider the report and to bring the matter to the notice of the American Foundrymen's Association at the next annual convention.

President J. S. Seaman, of the national body, reported that the convention would probably be held in Chicago. Boston foundrymen served notice at the last convention that they would like to entertain the foundrymen this year, but it has since been learned that next year would be more agreeable to them.

REVIEW OF THE FOUNDRY LITERATURE OF THE MONTH.

IRON AGE.

Touching upon the greater demand existing for charcoal iron at the present time as compared with that of a short time ago, this journal says:

The revival in the demand for Lake Superior charcoal pig iron and the good prices now ruling are not merely accompaniments of the general prosperity. They are not due alone to the act that the supply of coke pig iron was barely equal to the demand for the greater part of last year and that anything in the form of pig iron was eagerly taken by hungry consumers. The better trade in charcoal iron is also due to the gradual return of many malleable casting manufacturers to the use by preference of that class of material. When all prices were extremely low, and makers of malleable castings professed to be thoroughly satisfied with results obtained from the use of coke iron, the future of the charcoal iron producer looked most unpromising. Unless a charcoal furnace could turn out its product at a cost close to that of coke iron, it appeared to be doomed to idleness and decay. As few of them were able to endure such a strain, it seemed at one time as though the fingers of one hand would be more than enough to count the number of furnaces sure to continue in the business of making Lake Superior charcoal iron. But quite a transformation has occurred of late in this branch of trade. Old furnaces that had long been regarded as abandoned are again in blast and others are being prepared for operation as rapidly as possible. The coming year promises to see a very substantial increase in the output, and from present indications it will be needed. For some time the supply of coke iron for malleable purposes has been short because of the demand for Bessemer pig from the steel works, and the malleable foundries would have been obliged to turn to the charcoal furnaces for at least part of their supply. But as favorable or unfavorable forces develop

in couples, if not in greater number, so it happens that just at this time a distinct tendency is seen among the malleable concerns to prefer charcoal to coke iron. This has proved to be especially the case with makers of small malleable castings, yet even those who turn out draw bars and other large sized malleables have, in certain instances, manifested the same disposition, using at least half charcoal and half coke as a mixture, giving more satisfactory results than all coke, as used up to a short time since. The great growth in the production of malleable castings offers the producers of charcoal pig iron an increasingly attractive field.

AMERICAN MACHINIST.

In the issue of March 1st R. H. Palmer illustrates the molding of a propeller wheel in green sand.

W. H. Booth, writing of "White Metal or Gunmetal for Bearings," says:

An experienced American engineer once remarked to me that the use of gunmetal in America had not been the success which it has always proved in England. It was not, he said, that its quality was bad, but there was some bar to its success which did not, apparently, exist in England. Of late years, however, in England I have noted a growing use of soft or white metals, and when the facts come before one it becomes necessary to seek the real causes at work. In trying to find such cause or causes it has occurred to me that twenty-five or thirty years ago there was comparatively little engineering carried on in America, whereas here it was an old-established industry. It is thus within the past thirty years that machine construction in America has grown up and found what may be described as the line of least resistance. Assuming, as we may very properly do, that the lines of least resistance in mechanical work are determined by experience and the general average of the results of trial, we find that one line has been found in the avoidance of gunmetal* and the

*Gunmetal is a term commonly used in England to signify bronze, while white metal corresponds to the class of alloys known in this country as babbitt.

adoption of white metal, and it has occurred to me that the period of development of American work has curiously coincided with another fact in engineering.

I have been casting back my thoughts to early days, and remember that in the stores of my father's factory there were two large tin cylinders of oil. One of them contained oil for common purposes, and the other sperm oil. I remember how this oil had become very scarce. We paid from \$2 to \$2.50 per English gallon for it (i. e., 8s. to 10s.). Light lubricating oils to-day may cost about 16 cents per gallon. Naturally, the oil question was then one of considerable importance. It cannot be denied that sperm oil was a first-class lubricant. Chemically speaking, sperm oil is a fluid wax. It is of low viscosity at 60 degrees F., and yet maintains its viscosity well as temperature rises—a fact held by some to account for its excellence as a general lubricant. Its specific gravity is about .875 to .884, whereas whale oil has a specific gravity of .921, and all other vegetable and animal oils are heavier than sperm oil. This fact made it impossible to adulterate sperm oil with other organic oils. But certain of the mineral oils of Russian and American origin are of even less specific gravity than sperm oil, and could not, if mixed, be detected by the method of the hydrometer. Mineral oils only came into use about thirty years ago, or less, in this country. We may assume that in America mineral oils were pretty firmly established before the growth of mechanical engineering had fairly commenced. Now I am coming to the fact that I wish to emphasize, namely, that English machinery work has grown up and been firmly established on organic oils, whereas American industry has been fostered on mineral oil. Both countries now use mineral oil to the large exclusion of the organic oils, though, excepting in steam cylinders, it is open to doubt if mineral oils alone are altogether desirable. With expensive sperm oil it was an object to find a substitute. In the general work of cotton mills I found that by mixing sperm oil with half its bulk of mineral oil, at half the price, a saving of about 7 per cent at least was effected in the total driving power. Thus, by reducing the cost of oil one-fourth the fuel bill was

reduced. Sperm oil, like other organic oils, will oxidize or gum, though to a less extent than most oils. The mixing with mineral oils protects the organic oil from the influence of air, and this possibly accounts for the improvement noted. It is now well recognized that mineral oils have this property. But mineral oils fail in respect to that property of greasiness which can best be explained by rubbing a sample of oil between finger and thumb. This deficiency in mineral oils has prevented their use pure with gunmetal or bronze bearings, and in England it has been customary to use blended oils, though, as a fact of long standing, pure mineral oils have been used in America, but with white metals. It seems probable and likely that the plentiful supply of mineral oils has thus practically led to the so universal employment of white metal in America, and is rapidly doing the same here.

Few realize how very serious is the loss of power from unsuitable lubrication. Were this better realized there would be more experiments made with a view to determine the really cheapest oil. This is determined quite as much in the coal heap as in the oil bill. Pure mineral oils are said by one authority only to be suitable for pressures below 70 pounds per square inch. It may be that the heavier pressures allowed here than in America are more or less a part of the same chain of circumstances.

We are all apt to criticise unfavorably proportions in machinery which do not fall in with our own ideas and experience. I fear we often do this without sufficient thought to the difference of conditions. If we cannot see such difference it is just as well to think it out.

In the issue of March 8th, R. H. Palmer dwells upon "Judgment in the Use of Cores," and writes:

The vexing question often arises in the foundry as to whose fault it is that a cored casting is poor. Often the molder says, "It's the coremaker's fault!" "There's too much flour in the core mixture." "The core's as hard as a brick." The gas don't escape, or it was not vented right, or not pasted properly, or there is something the poor coremaker is to blame for, and he (the

molder) is in no way responsible. It's that other fellow every time. Now, in many cases the above is true. Many foundries have a man in their core-room who mixes all of the sand made into cores, and they find that it pays better to do so than to allow each coremaker to mix his own sand. Under this system, when a coremaker is given a box he is told to make the core from a mixture in such a bin. The man doing the mixing has his measure to measure sand and flour, and has no excuse for not doing as he is told, while the coremaker does not have a chance to guess at the mixture, because the measure is not handy.

One of the things that mean a great deal to the molder in saving time is to have his cores ready for him to set as he closes up his molds—that is, when it is possible to set cores at that time. In snap work of course it is a necessity.

But I refer now to tight flasks and side floor work. We will suppose the cores are ready. Sometimes a man has a job where if he could set the core when he finished the mold, it would save an extra lift; but the question comes up—will not the core get damp and blow?

If a core is some 10 inches between prints and, say, $\frac{3}{8}$ -inch diameter, and is made of flour and sand, it will stand the iron rising around it if it is not left in the mold too long before pouring, or if the core stands upright; but a molder would expect to have a rod in it if he were to set it in the morning and leave it all day; and if the iron were to enter the mold in such a way as to flow partly against the core, a rod would be necessary anyway.

Now, were the same size core to be set lying on its side, a rod would be necessary to strengthen it to stand the upward pressure of the iron flowing under while pouring, and the more the strain the stouter the rod. If the core is set too soon in the mold, and especially if a little warm, it will collect the dampness, and will become very soft. I have seen men go into a core-room, pick up a core and try to bend it, and remark: "That's stiff enough; that won't spring." Yet, taking the same core and leaving it in the mold a little too long before pouring, so that it gets damp, it loses its rigidity, and springs. When dry, the

mixture of flour and sand around the rod makes a core with even a light wire in it very stiff, backing up, as it were, the wire; but when damp, and not so damp that it will blow to spoil a casting (as far as any kick from it comes), the core will be sprung and the hole crooked.

Are there not many castings lost by cores being set in the mold too early in the day, and thus getting too damp?

Once I had quite a number of castings to make off a pattern requiring a core some 9 inches long, some 7-16 inch opening, and, after entering the casting, some $\frac{3}{4}$ inch at each end, swelling out to form a chamber, and the casting was cast with the core lying down. I gave the corebox to the coremaker, with instructions to make so many per day, and to the molder the pattern, with "Make, say, eight flasks a day. Look out you don't set the cores too early." The first day's castings were all right; the second day's were blown. On seeing the coremaker he said, "The first day they were ready, and set just before the wind went on. The second, they were ready the first thing, and the molder took them." Seeing the molder: Yes, he set them just before the wind went on the first day, and the second he set in the morning the first thing, and he had them up now with the cores in. I told him: "Let them set." In a short time he came to me with "They tell me the cores are blown in those castings; I told that fellow he was pouring too much flour in, and I don't believe those vents were clear. I know one or two were poor. I have them in extra to-day." I merely said, "Very well, let them set." Just before the wind went on I went to this man, and had him lift the copes. The cores were so damp from the mold, they were ready to fall to pieces. Setting fresh cores, they were poured during the heat, and the castings were all right.

Because you can set a core standing on end, and have it stand the pouring all right, it does not follow that the same rule applies to cores lying down; and because you can set a core lying on its side in the morning that is 2 inches in diameter, it does not follow that you can set one $\frac{5}{8}$ inch diameter at the same time.

Rosin cores will stand in some ways more than flour, and small cores made with oil and baked well will stand dampness.

That it's the cores and the poor fellow's carelessness we know many times, but it's the poor fellow's carelessness who sets the cores that the poor fellow in the coreroom is often blamed for.

Sometimes it costs to find out some little points. Some years ago I was making some castings, two in a flask, requiring cores some 14 inches long, 3-16 inch on the small end, 7-16 inch diameter on the large, ranging from that up to 24 inches long, $\frac{3}{8}$ inch small, $\frac{5}{8}$ inch large end, and using 120 to 160 cores a day. After molding all up, the cores were removed and the cores set. Grasping a handful in my left hand, I would straddle the row of flasks, and picking up one by my right hand, glance along it to see if it was straight and the vent clear. If it was crooked I set the rounding side down. If it was straight I bent it, as it could be done on the smaller sizes, and the upward pressure of the iron straightened the core. I left these. Meeting the foreman of that shop one day on the street, he asked: "How was it you never had trouble with those cores? Every once in a while we have some crooked core-holes, but still the cores are good and straight. I looked them over several times." I said, "Why, my dear sir, I never set a straight core in all the thousands I used." "What? Well, I must be going." He caught on.

Again, if dull iron was used and the iron forced in, it would spring the cores. (The castings were bolsters for a speeder frame, or I think in cotton machinery they are called slubbers.) But with sharp iron poured in gently, giving a chance for it to flow easily, there was no trouble in coming dirty or cores springing.

A roll some 7 feet long, 5 inches diameter, with openings some $1\frac{1}{8}$ inches on the end, gave some trouble by the core springing. Chaplets could not be used; the roll was not more than 7-16 inch thick, as I remember; but by gating it the full length on both sides, and pouring with some eight hand ladles, four on each side, so that the iron quickly filled the mold, there was no trouble.

Many times, where possible, it will be found very beneficial if one end of a flask which contains a casting with a long and

comparatively slender core in it can be raised while pouring. In this way the strain is lessened, as the iron does not flow up under the core the whole length at one time. And many times if the iron is allowed to flow gently in starting with a certain sized stream, and keeping it that size, the casting will come out clean; but if a small stream is started, and then, after pouring a short time, the stream is increased, it is like the river, throwing drift-wood ashore by the normal current and by a freshet it is whirled into the stream again, and lands perhaps in the wrong place, and it is so with dirt in molds.

IRON TRADE REVIEW.

In reviewing "The 'Baby Bessemer' Process" and an article recently appearing in a German journal concerning same, Richard Moldenke says:

It will be remembered that not a great while ago strong efforts were made by interested parties to introduce into this country two French applications of the Bessemer process for making small steel castings. So far the results have not been very brilliant, for one process—the best one, by the way—seems to be dead, and the other has to contend with troubles of a kind peculiarly inherent to the American demand for success under adverse circumstances. Be it noted that the processes themselves, if completely carried out, give a uniform and good grade of steel castings, but—and here is the difficulty—they are not carried out as they should be, not even in France, as the tests made by the reviewer on the product have amply shown him. What can we expect from the American demand for tonnage made by cheap men under ordinary foundry management with processes involving the loss of about 17 per cent of the iron charged during the blow? No wonder that in this country the open-hearth steel process is rapidly encroaching on the field formerly covered by the Bessemer only. As the "baby Bessemer" processes are rapidly extending in Europe, however, possibly because they get a high price for the castings, the article to be discussed may be of interest.

From the historical standpoint it seems that the first attempt to make steel castings by the Bessemer process was made in Upper Silesia in the early seventies, thin steel plates for street paving purposes being cast direct from the converter. The process did not take in Germany until 1890, when the works at Haagen bought the rights for the Walrand-Legenisel process in Germany, and in 1896 licensed Mr. Carl Rott, of Halle, to undertake the production of Bessemer steel castings. Mr. Rott is the author of the article under consideration. England, Sweden, Russia and Austria, and especially France, went into the baby Bessemer industry with vigor. The process of Robert makes use of a side blast. Tropenas blows the air on top of the bath, while Walrand uses the ordinary bottom blast and much smaller charges than the others. At Halle, where the Walrand-Legenisel process is used, they have a converter in which about 800-pound charges are blown at a time. The iron, melted in a cupola near by, is run directly into the converter by means of an extension to the spout. The composition of the pig iron is as follows: Silicon, 2.3 per cent; carbon, 3.5 per cent; manganese, 0.8 per cent; phosphorus, 0.07 per cent. Ten per cent scrap from previous casts is added and the melting ration is 1 to 12½. Before the first charge is blown the converter is heated up with some coke burned under a light blast, then clean out and at once put into service. The process itself is interesting from the chemical standpoint, for the burning out of the silicon, manganese and carbon is to some extent regulated by the temperature of the bath attained. The iron from the cupola arrives at about 2,300 degrees F. and the conversion must raise this to about 3,100 degrees F. Now neither carbon nor manganese will furnish the fuel for this high temperature, but silicon does, every per cent of silicon thus burned being equivalent to a rise in temperature in the bath of over 500 degrees F. As the temperature gradually rises the point is reached (2,550 degrees F.) at which carbon will combine with oxygen before the silicon can get it, and hence the silicon will not go quite so fast while the carbon is reduced very rapidly. The German, English and Swedish adaptations of the Bessemer process are based

upon this phenomenon. Thus the German pig irons being fairly high in silicon, there is consequently a very high temperature in the converter. The burning out of silicon practically ceases at 2,550 degrees F. The carbon now burns out, leaving about .30 silicon in the bath. Ferro-manganese being now added, the after-blow brings the bath to the proper composition and temperature for casting. The German process of Bessemerizing is therefore characterized by the high initial temperature and the recarburization.

The English process deals with irons much higher in silicon and manganese, hence it is possible to blow with an initial temperature much lower than the German. Nearly all the silicon and manganese is burnt out in the first six minutes, the carbon now goes and with it the balance of the silicon, for the burning of the carbon tends to lower the heat of the bath slightly. In 12 minutes the 3.75 to 4.00 per cent of carbon is gone, and spiegel is added to get the proper composition. In the Swedish process the irons are so low in silicon that it must come from the cupola very hot. The burning of the silicon is just sufficient to reach the point at which the carbon can go, and not enough to allow recarburization. It is therefore necessary to stop the process at the exact carbon contents and cast at once.

In the application of the Bessemer process for small steel castings it is necessary to get a very high casting temperature, and hence Walrand adds 5 to 10 per cent of melted ferro-silicon to the bath after the carbon is gone. An after-blow, lasting about one minute, is given, and then the final $\frac{3}{4}$ per cent of ferro-manganese is added. The addition of the ferro-silicon and the after-blow results in a very high casting temperature. The addition of ferro-manganese results in a boiling up of the mass of steel, which subsides as soon as the reaction has taken place. Right here will be found the success or failure of the blow. There being little time to spare before the cast must be made (in the O. H. the metal should be in proper shape when tapped), it is difficult to get rid of the gasses completely. A test is made as soon as the bath is quiet, by pouring some of the steel into a

small ingot mold. If the metal solidifies and sinks it is in proper condition; if it boils over there is too much gas left in it, and more silicon, manganese or aluminum must be added. As this is often too late the result is a miscast, which goes to profit and loss.

Mr. Rott now gives some recipes for making the molds, among which are the following: (a) 5 parts ground crucibles, 1 part clay; (b) 6 parts ground fire brick (Schamotte), 4 parts molding sand, 4 parts river sand, 2 parts clay; (c) 8 parts river sand, 1 part ground coke, 2 parts ground fire brick, 2 parts clay; (d) 6 parts ground fire brick, 6 parts river sand, 2 parts ground coke, 3 parts calcined clay, 2 parts raw clay. The material is ground fine, well mixed and sifted. It is now spread in thin layers, dampened, and let stand 24 hours. It will now ball properly and can be used at once, preferably as a facing, old material filling up the balance of the mold. The molds must be well vented and properly arranged for casting, the idea being to distribute the fluid steel as quickly as possible, so that sound castings may be the result. A layer of fire clay and graphite is now dusted on and slicked off, the molds are dried carefully and while still hot coated with a wash of ground quartz in molasses water. Green sand castings are made in molds rammed up with sharp sand mixed with a little coal tar. Great trouble is experienced from the presence of water in the blast; this got so serious that just as the carbon burnt out great flames issued from the converter; and a burned, gassy, and weak steel resulted. After settling tanks and screens were provided this evil ceased to cause any trouble. Mr. Rott seems to have had trouble with his cupola, the iron chilling too quickly. He therefore suggests the addition of a hearth so that it can be kept hot for a long time by a species of refining. Finally Mr. Rott gives some costs which will interest us sufficiently to be given in full. A day's run of five blows of 770 pounds each, or 3,850 pounds, gave a net weight of castings amounting to 1,760 pounds. These castings cost:

For Labor—

Melting	\$ 2.40
Converting	1.60
Molds and cores.....	23.90
Cleaning	6.00
Labor	3.10
	<hr/>
	\$37.00

For Material—

Pig iron	\$17.36
Ferro-silicon	3.16
Ferro-manganese	2.54
Fuel	6.20
Supplies	4.44
Shop cost	11.00
	<hr/>
	\$44.70

This is on a basis of 17 per cent loss of pig iron, 6 per cent loss of ferro-silicon and 3 per cent loss of ferro-manganese, pig iron costing \$19.10 per ton. The total cost was therefore \$81.70, or 4.65 cents a pound. The castings weighing from $\frac{1}{2}$ pound to 23 pounds each, brought 6.82 cents a pound and over in the market, figures that are certainly of interest to us. Mr. Rott now enumerates the advantages of the baby Bessemer process over the open-earth methods, which, however, count for little with us here, as we are not over-fond of the Bessemer products anyhow, and concludes that a great future stands before this branch of the manufacture of steel castings.

Stuart B. Marshall contributes an article on "Analysis in the Foundry" as follows:

The value of silicon in iron for foundry use has undoubtedly been overestimated, for some seem to think it to be the one element which makes iron soft and saleable; whereas it is a softener and hardener under different conditions. Too much silicon in the iron will certainly make it weak and will often cause shrink-

age and drawing in castings, similar to sulphur. It will be of very little value in any iron unless there is enough total carbon in the mixture, for it seems to have the property of creating graphitic carbon and thus, under certain conditions, having indirectly a softening influence. This is why the name softener is used for irons having such a large percentage of silicon, but as is well known by most foundrymen you must not use too much or it will have just the opposite effect. For large castings, plates, etc., it is a great mistake to have too much silicon in the mixture, for they will be weak and porous. A moderate amount of silicon in pig is very desirable, and for most kinds of castings 1.30 per cent to 2.00 per cent is about right. Of course, if small castings which are to be machined are desired, then it is best to use a higher silicon iron, but it must be done with care. In our cupola at Dunbar we have used an iron containing 1.45 per cent silicon with great success on a variety of castings, and it will carry considerable scrap also. We have used foundry iron containing .90 per cent silicon for larger castings and it worked all right. Of course, the sulphur was low in these irons, and the carbon well up. So often the iron gets blamed for being hard, when in reality it has been caused by too much blowing, which simply blows out a great percentage of silicon and increases the combined carbon, also decreasing the graphitic carbon content; so the foundryman asks for a higher silicon iron, when, by proper attention, the iron he has will work all right.

Sulphur is a very undesirable element in foundry work, and the pig used should have a low percentage of it, always under .050 and preferably under .035, because an excessive amount of sulphur causes "red shortness" and shrinkage. Of course, the coke and scrap are to be considered more when it comes to looking up the sulphur, as the former will run about .70 per cent and upwards, sometimes very much higher; and the scrap is liable to run almost any percentage of sulphur, very often being as high as .15 per cent and over. There is hardly any more injurious element in foundry practice than sulphur, and, while it will make iron hard and cause it to chill, if you desire that kind of casting, the result

can be secured by reducing the silicon content and by more blowing. You can get a harder iron by increasing the combined and reducing the graphitic carbon. Sulphur more than any other element is the cause of so many complaints and one ought to be very careful as to the quality and quantity of scrap used. This precaution also applies to the coke. The pig should be tried alone with the coke so as to find out just where the trouble occurred. An excessive amount of sulphur seems to cause an increase in combined carbon with a decrease of graphite, which gives you hard castings, thus making them difficult to machine, showing how largely the low sulphur and high graphite content in foundry pig enters into the question of softness.

The importance of graphitic carbon in a foundry mixture can hardly be overestimated. A proper amount of that element is necessary so that the iron will run well, and be easily poured. A good percentage of graphitic carbon and low sulphur—and they usually are associated that way—are the first essentials of a good soft iron. And when they are in the pig in the proper amounts, the silicon does not cut so much of a figure, as you can make castings which can be easily turned, without so much silicon, and thus get a stronger and more homogeneous one; for too much silicon will make the castings dirty, too open and weak. From 3 per cent to 4 per cent is a good amount of graphite for foundry iron, and one must remember that old burnt scrap will work just the opposite way, and if used must be mixed properly. A great amount of scrap can be used if attention is paid to the mix, and one appreciates the variety of castings which are to be made; for what is good in one case will not work at all in another. Graphitic carbon seems to be the element, more than any other, which makes the pig and casting soft and easy to bore. It also favors fluidity and easy pouring.

Combined carbon seems to make the iron harder, more brittle and difficult to bore. It should be under .50 per cent in foundry iron unless very hard castings are needed or class of work being made which is to be very close and not to be machined. Even when the pig is not high in combined carbon, excessive blowing

will create it in the mix at the expense of graphitic carbon, and I have seen good soft iron condemned just because the foundryman was blowing the silicon and graphite out of the mix and increasing the combined carbon content. The resultant was a hard casting, about as hard as steel, which could not be turned or filed. In this case the loss of some silicon could have been borne, if the graphite had remained the same.

Phosphorus is supposed to make the iron more fluid, if present in a noticeable amount, and it certainly does by reason of being a heat agent, but you can get fluidity with a low phosphorus iron by having a reasonable amount of silicon along with high graphitic carbon and low sulphur content. For certain kinds of castings, where the iron has to run into small spaces, and for thin castings where there will be a considerable time consumed in pouring, it is well to have increased percentage of phosphorus, but when you have more than 1 per cent in the pig it makes it weak and one must look out for "cold shortness." It is better, in general practice, to keep the phosphorus under .75 per cent in the pig, as you will have a much stronger iron.

A moderate amount of manganese in the pig will not be harmful for castings, but it is well to keep it under .75 per cent, for, like sulphur, it will cause hardness, and unless you want that or chill then it is not necessary, for it will simply make the castings so hard that they cannot be machined. Sulphur has quite an affinity for manganese, and the presence of manganese also seems to favor the formation of graphite, under certain conditions only; because in the presence of high manganese I have seen the graphite reduced somewhat.

A brief summary of the above shows that conditions cut quite a figure with the elements in pig iron for castings. A foundryman should not condemn an iron until he has investigated the scrap, coke, the mixing and blowing, for very often the one thing he might pass unnoticed will be the one to cause the trouble. Too much silicon, sulphur, manganese or combined carbon will make the iron hard. Too much phosphorus makes it weak, so if strength is desired, a low phosphorus is necessary. In using heavy scrap

the silicon ought to be increased, but if there is not enough total carbon, it will be useless. Silicon and phosphorus are both hardeners and weakeners and do the work of the carbon at a cheaper cost. Grading by chemical analysis should be the only true selling guide, and while it does not yet seem to have been perfected, yet a scale made up on the silicon, sulphur and graphite contents ought to help the matter considerably. There is no use going by fracture, because it will mislead you as often as it will help, and this will be especially true of iron made under abnormal conditions.

I have seen iron with only .50 per cent silicon break with a beautiful No. 1 fracture and then again a 2.50 per cent silicon iron break with only a mill fracture. Both irons were under .030 per cent sulphur and over 3.00 per cent graphite. I have also seen men who judged by fracture pick out the very worst foundry iron on the yard just because to their so-called experienced eye it looked better, when in reality it was not so. When you see the peculiar changes of fracture in iron of the same cast and also in the same bed, one had better take up a more modern and scientific idea and rely more on chemical facts, which have proved to work better than the "rule of thumb" way; when taken up intelligently. The foundryman should work more with the furnaceman and let him know of his needs, and also when the iron works all right. As it is the furnaceman only hears of troubles, which, if the matter had been gone into before, could very likely have been obviated.

In conclusion, I will give an experiment which I made to show up several of the points which have just been mentioned. Two irons were tried separately for about two weeks on radiator work, which, as is well known, needs a very fluid iron with little shrinkage and a dense gray casting, which can be easily bored. The iron was charged in the cupola in the following proportion: 600 pounds pig and 400 pounds radiator scrap. The first test was with an iron of the following analysis: Silicon, 1.83 per cent, sulphur, .026 per cent, phosphorus .301 per cent, graphitic carbon 3.17 per cent. The radiators seemed all right in every respect. The next test was made with an iron of the following analysis: Silicon 1.00 per cent, sulphur .026 per cent, phosphorus .261 per

cent, graphitic carbon 3.09 per cent. The radiators were decidedly satisfactory, just as much so as in the first test, and the strange part was that the shrinkage was really less. In both cases the shrinkage was less than $\frac{1}{8}$ inch to the foot. The iron ran fluid in both cases, but the radiator from the 1 per cent silicon iron was decidedly stronger and denser. Both were easy to bore.

Touching upon the points brought out in the discussion of his paper on "Annealing Malleable Cast Iron," read before the Foundrymen's Association, Mr. Davis replies in the issue of March 15:

Replying to Dr. Moldenke's criticism on my paper, I wish to say that the castings mentioned in Table No. 1 were made from a mixture containing pig iron only; their total carbon was therefore higher than that of the bars mentioned in Table No. 3, which were made from a mixture containing 30 per cent of sprue and scrap. The castings were kept at an annealing heat four days for the first anneal, and at so high a temperature that the packing caked. These castings were of irregular shape and on this account it was out of the question to give exact carbon analysis. The thickest part of the castings was 3-16 inch, and the second annealing was given to see if there was a gain in the weight after practically all the carbon had been removed. As to Dr. Moldenke's criticism of the carbon analysis, I stated in my paper that the sample represented the entire cross-section of the bar. The method employed was to break the bar and then to file across the broken end, the bar being held in a vise and the filings collected on a large paper held underneath. I much prefer this method to drilling, as I believe a more homogeneous sample can be obtained.

As to the gain in weight: An examination of Table No. 3 will show that the largest gain was on the $\frac{1}{4}$ -inch bar. I think that even in a jobbing foundry running on light work $\frac{1}{4}$ -inch would come nearer representing the average cross-section of the work, and on the bar of this thickness the gain was 3.77 per cent. When we come to heavy castings such as couplers and other railroad work running for example, from $\frac{1}{2}$ -inch to $\frac{3}{4}$ -inch, we have a gain

as shown in Table No. 3 of about 2 per cent. I should state that the bars were kept at a moderate annealing heat three days. I have been told that it is the general practice in the Central West to anneal from one to two days. If this is a fact, I believe it is a shorter time than is the general practice in the East and it is a reasonable inference that castings annealed under such conditions would show a smaller increase in weight.

Now, as to what causes the increase in weight: Both Stead and Royston writing in the *Journal of the British Iron and Steel Institute* has shown that there is oftentimes a small amount of oxygen in the outer skin of a malleable casting. I have not a copy of Mr. Stead's original article (*Jour. Cleveland Ins. of Eng.*) before me, but if I remember correctly his analysis referred to a scaly casting. This scale, which was .012-inch in thickness, consisted of iron and crystals of silica. He stated that the silica was in the same equivalent proportion as the unoxidized silicon in the center of the bar. As this bar was $\frac{3}{8}$ -inch in diameter, it is evident that the proportion of oxygen present in the scale was very small. Royston speaks of finding a considerable amount of oxygen in the outer layer, but does not say how much or what proportion of the entire bar this layer represented. I have never been able to detect enough oxygen to account for more than an insignificant proportion of the gain in weight. One question arises: If no iron from the packing is reduced and welded on to the castings, what becomes of all the scale and iron or steel turnings that are used. Many malleable concerns have been buying scale for years and have no more on hand now than they had some years ago. Of course, there is a trifling loss in handling, but I don't think this would account for the disappearance of so much scale.

As to the "white crystalline rim" that Dr. Moldenke speaks of: These bars did not show it, except, of course, on one side where the metal was compressed by bending them in order to break them; but it was not a white edge such as is caused by too low silicon or too high sulphur in a casting. As to the diffusion of sulphur in this case the packing contained much more than the

casting and naturally the latter gained by the diffusion. If we had a case where the packing contained less than the castings, it is reasonable to infer that the latter would lose.

N. W. Shed, in the issue of March 22, takes up the same subject as follows:

In the recent paper on "Annealing Malleable Cast Iron," Mr. Davis shows that the iron gains from 3 to 6 per cent in weight. This he asserts is due to iron reduced from the packing and welded on to the iron casting. Dr. Moldenke criticises this and contends that the gain is due to oxygen and not to iron. To see if Dr. Moldenke's criticism was just, I selected a casting which gave signs of much oxidation and determined the oxygen and found seven-tenths of one per cent (.7 per cent) present. The average gain in weight of these castings is over 3 per cent, and this small amount of oxygen does not account for the gain. It is true there is more or less oxygen with the iron on the surface of the castings. This sometimes forms a beautiful purple coating, but it is insignificant when compared with the gain in weight.

The conditions of annealing give every reason to believe that Mr. Davis is right in his assertion. First. The packing in use contains a large amount of minute iron pellets. These may be separated with a magnet. Second. Most of the oxide of iron in the packing is the lower oxide. Only 9 per cent of sesqui-oxide is found in much packing. Third. There is sufficient carbonic oxide produced by the oxidation of the carbon diffused to reduce the iron oxide. Fourth. The heat is ample to effect the reduction and weld the particles to the annealed casting. The heat conditions are very similar to the heat conditions in roasting ore with culm in kilns. At the red heat of the kiln much iron is reduced and large pellets of metallic iron are found in the roasted ore.

Apart from this distinct gain in weight due to the accretion of iron is the phenomenon of expansion, always remarkable in annealed iron. At the heat of the oven the dense white iron, having a specific gravity of 7.8, loses part of its carbon and nearly

all the rest goes out of solution as graphite. An entirely new crystallization sets in and the casting expands like water in freezing. The iron now has the lower specific gravity of 7.6 showing spaces between the crystals; the graphite appearing in these spaces. If the anneal is carried further the graphite disappears and the annealed casting has many of the properties of wrought iron.

Our thanks are due Mr. Davis and Dr. Moldenke for calling our attention to this most common but most wonderful cast iron.

THE FOUNDRY.

R. H. Palmer contributes an illustrated article on "Reverberatory Furnaces."

Edward Kirk presents an exhaustive article on foundry irons and the characteristics of each.

Eli H. Pearce takes up the subject of "Men and Management," showing that the personal element enters largely into the success of manufacturing establishments.